Summary

This paper focuses on improving the representation of multilayer mixed-phase clouds (MLCs) in the Arctic using the ICON model, a topic of significant importance given the critical role these clouds play in the Arctic climate system. By analyzing two case studies, the authors investigate the effects of various microphysical parameterizations and introduce a new immersion freezing parameterization based on their measurements. Their work aims to address the persistent challenges in simulating the mixed-phase character of these clouds, advancing our understanding of the factors influencing cloud formation and phase partitioning in the Arctic.

The study identifies key shortcomings in existing modeling approaches. Notably, the 1-moment microphysics scheme significantly underestimates ice concentrations, while the 2-moment scheme requires a dramatic increase in ice-nucleating particle (INP) concentrations - by a factor of 10^6 - to match observed ice mass concentrations. Introducing secondary ice production (SIP) processes, such as ice-ice collision breakups, could provide a solution; however, scaling these processes realistically diminishes their impact on ice crystal numbers, suggesting potential gaps in the current parameterizations.

Additionally, the authors find that increasing grid resolution beyond 1.6 km does not improve simulation accuracy, as large-scale biases dominate over resolution effects. Similarly, reductions in cloud condensation nuclei (CCN) concentrations yield negligible effects. These findings underscore the need for improved SIP representations to enhance future Arctic ice simulations.

General comments

Assessment of the new immersion freezing parametrization

The authors do not clearly state whether they recommend the general use of the new immersion freezing parameterization in the Arctic or discuss its quantitative impact on SIP. A setup testing the magnitude of SIP with the original INP parameterization is missing. Including such a setup would help clarify whether the introduction of the new immersion freezing parameterization (without scaling) alters the impact of SIP, or if an accurate representation of INPs is not essential for modeling MLCs under the conditions studied.

Revisiting the role of INPs and the importance of SIP

The conclusion that warm-temperature INPs are crucial for modeling MLCs is not convincingly demonstrated, as this is only observed under unrealistically high INP concentrations (e.g., 1E4 and 1E6). Instead, the results suggest that SIP may be the dominant missing factor.

Critical discussion of SIP processes

While the focus on SIP's role in phase partitioning is valuable, the study lacks critical discussions that would provide a more balanced perspective:

1. **Ice-Ice Collisions**: The breakup parameterization for ice-ice collisions only shows an effect in its original version, which is acknowledged to be unrealistic. The implications of this limitation should be explored more thoroughly.
2. **Droplet Shattering:** This process is mentioned briefly but dismissed as not very relevant. However, the original parameterization is based on limited measurements and may underestimate its potential impact. Recent studies (e.g., (Lawson et al. 2023)) suggest that droplet shattering (DS) could be significant in the Arctic, particularly at higher temperatures. Could the poor representation of DS in the model mask its true importance in explaining observed ice concentrations?
3. **Cloud condensation nuclei scaling and SIP**: The study finds that scaling cloud condensation nuclei (CCN) to match observations increases rainfall but has minimal other effects. Yet, larger droplets from scaled CCN could enhance DS, potentially influencing SIP. This connection deserves further exploration.

Summary

In summary, while the study contributes to understanding Arctic MLCs, it would benefit from:

- Clarification on the general applicability of the new immersion freezing parameterization.
- More rigorous analysis of SIP processes and parameterizations, especially the role of DS.

These additions would enhance the paper's impact and provide a more comprehensive understanding of the factors controlling phase partitioning in Arctic clouds.

Specific comments

Clarification of ice metrics

It is unclear at times whether the authors are discussing ice crystal number concentration or ice mass concentration. In the abstract, the INP concentration is stated to need an increase by a factor of 10^6 to match observed ice mass concentration, while the ice-ice collision breakup increases cloud ice number concentration by a factor of 10^5. These two metrics are not directly comparable. Is SIP increasing ice crystal number concentration, ice mass concentration, or both? Please ensure consistent and precise terminology throughout the manuscript.

Inconsistent modeling approaches across case studies

The authors use a wide variety of setups and parameterizations for the third case study but do not apply a similar approach to the first case study, despite both suffering from the same fundamental issue - the lack of a mixed-phase character in the base experiment. Why weren't additional setups tested for the first case study? None of the setups used for the first case resolves this issue, and further exploration seems warranted.

Terminology for INP scaling

The term *INP perturbation* is somewhat confusing. Would "scaled INP" be a clearer and more accurate description? Additionally, the terminology is inconsistently applied; at times, it is referred to as *tuned* or *polynomial scaled*, as seen in Figure 1. I recommend consistently using "scaled INP" throughout the paper for clarity and uniformity.

Abbreviations for scaled warm INP

For the polynomial-scaled warm INP, the authors sometimes use terms like *INPx10*^{4} or *1E4* but at other times refer to it more generally as *perturbed INP concentration*. To maintain consistency, I suggest introducing and consistently using abbreviations such as *1E4* and *1E6*. The introduction could be done in Section 3.1.2, "Primary Cloud Ice Formation". This would align with the introduction of the scaled CCN parametrization in Section 3.1.1. This would improve clarity and make it easier for readers to follow the paper's content.

Underuse of the second case study

The data for the second case study is minimally utilized in the paper, with only a couple of sentences dedicated to it. If it does not contribute substantially to the conclusions, consider removing it entirely and focusing on the first and third case studies. This would improve the paper's focus and streamline the discussion.

Specification of model and schemes

The manuscript does not specify which version of ICON was used or whether the implicit or explicit two-moment microphysics scheme was employed. Given that the implicit scheme is the default, it is likely the one used, but this should be clearly stated in the text to avoid ambiguity.

Public accessibility of data

To support transparency and reproducibility, the data used in this study should be made publicly available. This is a crucial step for enabling further research and validation.

Technical corrections

1. **Line 46**: The term "successfully" seems overly strong in this context. In contrast, Korolev and Leisner (2020) emphasize the need for further laboratory studies to obtain a quantitative understanding of the efficiencies of individual SIP mechanisms. Consider rephrasing to reflect the ongoing challenges and uncertainties in this area.

2. **Line 57**: The phrase "cloud ice concentration" should be replaced with "cloud ice number concentration" for precision. As written, "cloud ice concentration" could refer to either number concentration or mass concentration, which are distinct quantities, and the sentence would not hold true for mass concentration. Apart from this, there are several other sentences in the paper, where it is not clear, if the authors talk about number or mass concentration.

3. **Lines 173–174**: The description of droplet shattering is misleading. It refers to the process where large supercooled droplets (typically >50 µm in diameter) fragment into smaller ice particles, *not* the freezing of rain droplets. Refer to Korolev and Leisner (2020) for more accurate details.

4. **Lines 230–233**: This sentence is overly complex and difficult to follow. Please split it into two sentences for better readability and understanding.

5. **Line 264 onward**: The authors mention an inversion predicted at 2 km, 1 km higher than observed. However, Figure 6d also shows an inversion predicted at 600 m, which is not discussed in the evaluation. This omission should be addressed.

6. **Line 336**: The impact of the CCN scaling is smaller compared to what? Please clarify by providing a complete sentence to specify the reference point for this comparison.

7. **Line 360**: The section title “Can the INP perturbation be explained by SIP” seems misleading. The term “INP perturbation” refers to an artificial adjustment introduced in the study rather than a physical phenomenon. Given the author’ss finding that INP concentrations would need to increase by a factor of 10^6 to match observations, a more appropriate question might be whether SIP can instead account for the discrepancy between modeled INP concentrations and observed values. Consider revising the title to reflect this more accurately.

8. **Lines 399–400**: The mention of the maximum ice number concentration enhancement ratio is unclear. Is this information presented in any figure? Figure 9 only shows ice water content, which does not provide insight into ice number concentrations.

9. **Line 400:** The abbreviation "QNI" is used but never introduced or defined. Please provide its meaning when it first appears.

10. **Figure 1**: To show all the evaluated setups in your study, consider including pr_{1L-1} in the figure, even if it only adds a single line to the plot. This would provide a more comprehensive overview of the setups analyzed.

11. **Figure 3**: Please clarify the unit in the figure. Are you referring to the sea surface ice fraction?

12. **Figure 4**:
- The base and cloud tops do not appear to align with the measurements of ice water content. Could you explain this discrepancy?
- Additionally, there seems to be a missing cloud top at 00:00 UTC on September 2. Please verify and address this.

13. **Figure 5**:
- Is this figure based on model output or measurements? Please clarify in the caption.
- The mean sea level pressure is difficult to read, and the ship location (marked with a star) is barely visible. Consider increasing the plot size and improving the visibility of key elements.

14. **Figure 10**: The markers for “SIP scaled”, “1.6 km”, and “INP” setups overlap, making the comparisons discussed in the text difficult to distinguish. Could you consider alternative visualization methods, such as using smaller markers or providing a zoomed-in view of this section? Additionally, are precipitation observations available that could be included for reference to further validate the model's performance?

15. **Section 4.1 Synoptic Situation**: The detailed description of back trajectories would be more effective if accompanied by the corresponding figure in the main text rather than in the appendix. This would improve the flow and understanding of the discussion.

References

Alexei V. Korolev and Thomas Leisner, 2020. *Review on experimental studies of secondary production.* Vol. 20, 11767-11797, Atmos. Chem. Phys.

R. Paul Lawson, Alexei V. Korolev, Paul J. DeMott, Andrew J. Heymsfield, Roelof T. Bruintjes, Cory A. Wolff, Sarah Woods, Ryan J. Patnaude, Jørgen B. Jensen, Kathryn A. Moore, Ivan Heckman, Elise Rosky, Julie Haggerty, Russell J. Perkins, Ted Fisher and Hill, Thomas. C. J., 2023. *The Secondary Production of Ice in Cumulus Experiment (SPICULE).* Vol. 104(1), E51-E76, BAMS.